

**RESPONSE** to comment 1 (uploaded 18 June 2008) of *Economics E-Journal* discussion paper DP 2008-18: *Forecast Evaluation of Explanatory Models of Financial Return Variability*

Response to **Introduction**:

*The aim of the paper is to develop a framework for forecast comparisons for volatility models of financial returns. In particular, in order to rank discrete time volatility models, the author considers frequently used loss-functions like the MSE or the MAE applied to forecasts of the conditional variability (approximated by the squared returns).* These first two sentences of comment 1 suggest two of the key concepts of the paper are mis-interpreted. In particular, I do not approximate conditional variability by means of squared returns (the squared relative price increment of a financial asset). Rather, I *define* variability as squared returns, and (the) conditional (expectation of) variability is thus a prediction of variability. Second, my main objective is *not* to compare volatility models. Rather, volatility is one out of the two components that make up conditional variability.

Response to **Comments**:

1. *It is unclear how general and relevant for practical purposes the results are; parameters should be estimated.* I refer to my response to referee 2, since I have already addressed these issues there: Point 2 in the general remarks part, and points 2 and 5 in the specific remarks part.

2. *It is unclear why volatility is not a variable in the theory mechanism or in the DGP.* I deliberately avoided this issue because it relies on philosophical arguments, so I am grateful for the opportunity to address the issue in more detail. But before I answer the question let me clarify the notion of a DGP as used in the paper (and as used by the GETS methodology). The notion of a DGP in the paper is used in its reduction-theoretical sense, see Hendry (1995, chapter 9) and Sucarrat (2007). From a reduction theoretical point of view there is only one DGP: *The DGP*. *The DGP* is the most accurate and complete probabilistic representation of the relationships between the variables in question, and *the DGP* is thus intended to serve as some sort of “probabilistic ontology” (a probabilistic representation of reality as it objectively is). Accordingly, the volatility models or continuous time structures that have been put forward in the literature can thus simply not be a DGP in the reduction theoretical sense, since they are not accurate and/or complete enough to constitute an ontology. However, they can constitute what I elsewhere call “estimation and inference” models (Sucarrat 2007, introduction and figure 1 in particular, but also the discussion in section 4.2 is relevant) or alternatively a *simulation DGP*. My simulation DGP (equation 5), note that I use the qualifier “simulation” throughout in order to distinguish my simulation DGP from *the DGP*, is an estimation and inference model that at best is a

congruent (or otherwise “true”) representation of *the* DGP. In other words, my simulation DGP is not a DGP in the reduction theoretic sense.

Now I turn to the question of why volatility cannot be a variable in *the* DGP. The answer is somewhat philosophical, and for simplicity of discussion (but with no loss of generality) let us abstract from measurement issues so that the DGP corresponds to the theory mechanism, see Sucarrat (2007, section 4.3) for a more detailed discussion on this issue. *The* DGP is intended to be something objective and our most accurate and complete probabilistic representation of reality (and possible realities) *independent* of ourselves and our representations of it. Unless you believe in a rather unusual version of Platonism, and Platonism itself is a questionable philosophical thesis, volatility as such does not exist objectively, that is, independent of the modeller. The reason is that volatility, from a reduction theory point of view, is a model of the unexplained portion of the mean and thus determined by the modeller through the specification and assumptions on the standardised residuals, and through the choice of the conditioning information. So volatility does *not* exist independently from the modeller and can therefore *not* be a variable in the DGP.

*3a). Explanatory variables are likely to account for a decreasing portion of return variability as the size of the time increments tends to zero, but it is not the case that variability also tends to zero as the time increment tends to zero?* This is correct, and I see now that I should have been more careful in my wording. To avoid misunderstandings I should have written: The explained portion reaches zero before the time increment reaches zero.

*3b). The author is in contradiction with the common view and should be more explicit in referring to the common view, for example Andersen and Bollerslev (1998), and Patton (2007).* This comment surprises me because I think I have been very explicit in addressing the common view. There are numerous references throughout the paper and in a sense the main purpose of section 2 is to explain and justify the differences with the common view. In particular, I believe I am very explicit and detailed in subsection 2.2 in referring to the relevant literature. Note in that regard that the results and contents of Andersen et al. (2001) and Andersen et al. (2003) are more general and powerful than those of Andersen and Bollerslev (1998), and that Patton (2007) is a newer and updated version of “Patton (2006)”.

*4. There is a conceptional problem with the sample kurtosis of the standardised residuals as a goodness-of-fit measure analogous to the standard error of regression in a constant volatility context. Rather, the (mean) squared forecast errors (MSFE) of variability constitutes a goodness-of-fit measure analogous to the standard error of regression in a constant volatility context.* I see the point that in a certain sense MSFEs also can be seen as a goodness-of-fit measure analogous to the standard error of regression. However, I do not agree that the sample kurtosis cannot and the paper studies both. As for the problems and advantages with the sample kurtosis, see my response to referee 2: Point 1 in the specific remarks part.

## References

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